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REPORT ON PHYSIOLOGY.*

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ANIMAL HEAT.

ROSENTHAL.—Zur Kenntniss der Wärmeregulirung bei den Warmblütigen Thieren. Erlangen. 1872.

LIEBERMEISTER.—Zur Lehre von der Wärmeregulirung. Virchow's Archiv, Bd. 52, p. 123.

ROEHRIG and ZUNTZ.—Zur Theorie der Wärmeregulation und der Balneotherapie. Pflüger's Archiv, iv. p. 57.

PAALZOW.—Ueber den Einfluss der Hautreize auf den Stoffwechsel. Pflüger's Archiv, iv. 492.

VIRCHOW.—Wirkung kalter Bäder und Wärmeregulirung. Virchow's Archiv, Bd. 52, p. 133.

SENATOR.—Untersuchungen über die Wärmebildung und den Stoffwechsel. Reichert und Du Bois Reymond's Archiv, 1872, p. 1.

WINTERNITZ.—Beiträge zur Lehre von der Wärmeregulirung. Virchow's Archiv, 1872, p. 181.

GARROD.—On the Relation of the Temperature of the Air to that of the Body. Journal of Anatomy and Physiology, vol. vi. p. 126.

HORVATH.—Zur Physiologie der thierischen Wärme. Centralblatt für die Med. Wiss., 1872, p. 706.

Zur Lehre vom Winterschafe, ditto. 1872, p. 865.

DRAPER, J. C.—On the Heat produced in the Body and the Effects of Exposure to Cold. American Journal of Science and Arts, December, 1872.

JUERGENSEN.—Die Körperwärme des gesunden Menschen. Leipzig. 1873.

We have, in the above-mentioned pamphlet of Prof. Rosenthal, an excellent *résumé* of the most important recent investigations in this department of physiology, and a clear statement of the present condition of our knowledge of the subject. This work is one of especial value to practising physicians, now that the thermometer has come to play such an important part in diagnosis and prognosis, for we find here a clear statement of the conditions under which alone accurate measurements of animal heat are possible. For a better comprehension of the problem, the author conceives the animal body as divided into three portions:—

I. A central portion, where the temperature is uniform, or nearly so, throughout.

II. An external layer, exposed to the cooling influences of the air.

III. An intermediate zone, where the temperature increases gradually from without inwards. The thickness of this zone differs in dif-

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ferent parts of the body, being greatest in the most exposed portions.

To determine the real temperature of the animal body, the thermometer must be passed through the intermediate zone, and the measurements taken in the central portion. This is best accomplished in the rectum, and in order to be sure that the instrument lies in the central portion, it must be introduced slowly and the temperature read in different positions. As long as the mercury continues to rise in proportion to the depth to which the thermometer is introduced, it must be regarded as lying in the intermediate zone ; but when a deeper introduction no longer has this effect, the reading of the thermometer may be considered as expressing the absolute temperature of the body. All the other parts of the body, which are used for thermometric purposes, viz., mouth, vagina, axilla, &c., lie in the intermediate zone, and give a temperature not only lower but more variable than that of the rectum. This is especially true of the axilla, whose temperature, being greatly dependent upon the vascularity of the skin, is sometimes found to rise under circumstances which cause that of the rectum to sink.

The constancy of the temperature of a warm-blooded animal is, of course, dependent upon an equilibrium being established between the production and the loss of heat in a given time. Any increase of the production or diminution of the loss of heat will therefore raise the temperature of the body, and, conversely, any diminution of production or increase of loss will lower it. The regulatory arrangements, by means of which the body is able to maintain the uniformity of its temperature in spite of variations of its surroundings, affect chiefly the loss of heat. Thus, exposure to cold causes contraction of the cutaneous bloodvessels, in consequence of which less warm blood is brought to the surface of the body, and therefore less opportunity offered for loss of heat by radiation. Exposure to heat has, of course, the opposite effect. The power of the body, however, to keep its temperature constant is confined within narrower limits than has been supposed. Rosenthal found, in his experiments on rabbits, that these animals could only preserve their normal temperature when the surrounding media had a temperature between 11° and 32° C. Man, who is able to adapt himself to a greater variety of climates than any animal, owes this power chiefly to his ability to effect voluntary changes in his food and clothing. Even in man, it has been found that a slight increase in the loss of heat, as by sudden removal of the clothes, is sufficient to cause a perceptible lowering of the temperature in the rectum.

There seems to be often a great want of promptness in the working of the regulatory arrangements, for Rosenthal found that, when a rabbit had been exposed for a certain length of time to air of 36° – 40° C., and then removed to air of ordinary temperature, the thermometer in the rectum indicated, for the next two or three days, a diminution of about 1° C. in the temperature of the body. When exposed to a sudden lowering of temperature in the surrounding media, we may " catch cold " in two different ways : firstly, heat enough may be abstracted from the warm surface of the body to cause a dangerous lowering of the temperature of the body ; or, secondly (if the cutaneous bloodvessels contract to prevent this loss of heat), a sudden collateral hyperæmia may be produced. The prophylactic effect of cold baths

seems to be due to their causing an increased "tonicity" of the cutaneous bloodvessels, so that they do not dilate so much under the influence of a high temperature of the surrounding media. Variations in the activity of the respiration and perspiration also influence the loss of heat from the body.

In addition to these means of regulating the *loss* of heat, are there any arrangements by which the *production* of heat can be varied in accordance with the needs of the body? In other words, does the production of heat in the body vary with the temperature to which the body is exposed?

This a question which, for more than ten years past, has been under discussion in Germany. The most conspicuous of the disputants, with their most recent writings, are given in the above list.

The affirmative side of the question is maintained by Liebermeister and by Roehrig and Zuntz. The facts upon which they rest their opinion are the following:—

I. A cold bath, or exposure of the skin to cold, often causes a temporary rise of temperature in the axilla or mouth. To this Senator and others object that measurements in the axilla do not give the true temperature of the body; that, under the same circumstances, the temperature in the *rectum* steadily falls from the beginning of the experiment; and that the temporary rise in the axilla is due to the constriction of the cutaneous vessels, in consequence of which the blood is not so much cooled in its passage through the limbs. The temperature rises, therefore, in the axilla in the same way as after the application of a tourniquet to the brachial artery.

II. The calorimetrical experiments of Liebermeister seemed to show that, by quietly lying in a bath of 20° – 30° C., not only the loss but also the production of heat is increased; so that in a bath of 20° – 23° C. from three to four times, and in a bath of 30° C. about twice as much heat is produced as by the same individual under ordinary circumstances.

The value of these experiments is disputed by Winternitz and Senator, who point out various defects in the methods adopted. Senator maintains that, in his own more carefully conducted calorimetrical experiments, no increased production of heat under the influence of cold could be detected.

III. Exposure to cold causes an increased absorption of oxygen and elimination of carbonic acid.

Senator admits the correctness of this observation, but concludes, from his own calorimetrical experiments, that heat production and excretion of carbonic acid do not always go hand in hand. He finds, for instance, during digestion, an increase both of carbonic acid excreted and of heat produced, but the production of heat is more increased than the elimination of carbonic acid. On the other hand, he finds that exposure to cold causes no increased production of heat, measured calorimetically, but a decided increase in the elimination of carbonic acid. Hence we must admit that there are processes going on in the body which produce heat, but do not set free carbonic acid; and, on the other hand, that the elimination of carbonic acid does not prove a contemporaneous production of heat.

Roehrig and Zuntz regard the production of heat in consequence of exposure to cold as a reflex phenomenon, dependent upon an irrita-

tion of the sensitive nerves of the skin, causing an increased metamorphosis of tissue. This same effect may be produced by irritating the cutaneous nerves in other ways, e. g. by salt water. In this way is explained the invigorating effect of sea-bathing.

(In this connection are to be noted Paalzow's experiments on rabbits, which show that a mustard poultice on the abdomen causes a decided increase in the absorption of oxygen and elimination of carbonic acid.)

Recognizing the important part played by the muscles in the production of heat, Roehrig and Zuntz regard the regulatory process as probably consisting in a weak reflex irritation of the motor nerves, which increases with the difference of temperature between the animal and its surroundings. Under ordinary circumstances, this irritation is not sufficient to cause a visible contraction of the muscles, but under the influence of severe cold we have the phenomenon of shivering, which, on this theory, is only an exaggeration of the normal regulatory process. If this view of the question be correct, we should expect that paralysis of the motor nerves by curare would not only lower the temperature of the body, but would also reduce the regulatory power to a minimum, and this is found to be really the case. On the whole, therefore, although an increased production of heat as the result of exposure to cold cannot be regarded as absolutely demonstrated, yet it must be admitted that there are many observations which point in this direction. Since we know, by the observations of Brondgeest, that muscular tonicity is a reflex phenomenon, it is reasonable to suppose that this tonicity will be increased by stimulation of the cutaneous nerves. This, of course, will cause an increase of the heat production which always accompanies muscular activity. On the other hand, as Rosenthal observes, the success of the cold-water treatment of fevers would hardly be explicable on the theory that an increased loss is necessarily accompanied by an increased production of heat.

The experiments of Horvath were made on marmots and hedgehogs, with a view of determining some of the characteristic phenomena of hibernation. The temperature in the rectum of a hibernating animal was found to sink as low as 2° C. On awakening from this condition, the temperature rose rapidly to 32° C.; but this rise was preceded (as far as can be judged from the meagre details of the experiments which are given) by an increased rapidity of the respiratory movements. Respiration experiments were made, both before and after waking, showing that the elimination of carbonic acid was four times, and that of water seven times, as great in the waking as in the sleeping condition. Experiments consisting in artificially cooling hibernating animals, showed that their hearts continued to beat and their muscles remained irritable at temperatures much below that at which these phenomena ceased in rabbits.

Draper's experiments had for their object, "to determine the quantity of heat passing off from the surface of the body, by finding how much it would elevate the temperature of a known mass of cool water during a given period of time." It was found that by lying quietly for one hour in a bath of 74° F., enough heat was lost from the body to raise the temperature of the water 2° F. and to lower that of the body, as measured in the mouth and armpit, 1° F. As the volume of water in the bath was seven and one-half cubic feet, and that of the body

three cubic feet, it was concluded that enough heat is evolved from the body in one hour to warm the body itself 5° F. Dr. Draper further concludes that "the converse of this may also be considered as true, viz., that after death, the air being at 73° F., enough heat is lost in the course of an hour to cool the body five degrees, at least during the first hour." This, of course, could only be true if the production and loss of heat in the living body were the same as in the corpse, and the conducting power of air the same as that of water. From the figures in Dr. Draper's tables, it can be calculated that in a bath of 74° F. (23.3° C.) the body loses in one hour an amount of heat equal to 231 Calories, i. e. an amount capable of raising the temperature of 231 litres of water 1° C. This loss is not so great as has been observed by Liebermeister and others in baths of similar temperature, and the cause of the difference is perhaps to be found in the greater duration of Dr. Draper's experiments. Thermometers in the mouth and axilla indicated a steady fall of temperature during the bath and for a short time after leaving it. This was accompanied by a diminished rate of respiration and pulse. The continuance of the effect after leaving the bath is not explained by Dr. Draper, but it seems reasonable to suppose that the evaporation of the moisture adherent to the body on leaving the bath may have caused a further reduction of temperature. The fact that the air in the room had a temperature of 90° F. and contained only 53 per cent. of the amount of moisture necessary for its saturation, lends force to this suggestion. The diminished rate of the pulse seems to be due to a direct effect of a lowered temperature on the heart's action as demonstrated by Cyon.* The slow respiration may also be due to the same cause, but it is important to bear in mind that the rate of respiration has no direct connection with the activity of the interchange of gases in the lungs, and also that in counting one's own respiratory movements (as Dr. Draper himself observes), it is "very difficult to avoid influencing them in the act of counting." The most important result of Dr. Draper's experiments is that, when the experimenter in the bath executes continued muscular movements, the temperature of the water around him is not raised any higher than when he remains perfectly at rest. We must therefore conclude that the application of water of 75° F. to the surface of the body reduces to a minimum the function of the skin, considered as a regulator of animal heat, "and the whole duty of exhalation of vapor of water and consequent removal of heat is thrown upon the lungs; hence the increased respiratory actions, and hence also the special tendency of application of cold to the surface to produce inflammations of those organs by increasing the work they are obliged to perform."

Jürgensen publishes in pamphlet form observations previously communicated to the *Deutsches Archiv für klinische Medicin*. According to this observer, the temperature measured in the rectum of a person at rest has a normal daily variation of about 1° C., the highest temperature being in the afternoon, and the lowest between midnight and morning. The taking of food raises the temperature slightly, but not enough to interfere with the regular diurnal variation. The connection of sleep with changes of temperature is unfortunately not spoken of.

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* Ueber den Einfluss der Temperaturänderungen auf Zahl, Dauer und Stärke der Herzschläge. Arbeiten a. d. Phys. Anstalt zu Leipzig, i. p. 77.

TEMPERATURE IN RIGHT AND LEFT VENTRICLES.

KOERNER.—Beiträge zur Temperatur-topographie des Säugethierkörpers. Breslau, 1871.

HEIDENHAIN.—Ueber den Temperaturunterschied des rechten und linken Ventrikels. Pflüger's Archiv, iv. 558.

BERNARD.—La chaleur animale. Revue Scientifique, i., 133 et seq.

STRICKER and ALBERT.—Untersuchungen über die Wärmeökonomie des Herzens und der Lungen. Stricker's Medizinische Jahrbücher, 1873, p. 30.

The observations of Bernard and Liebig were for a long time regarded as conclusive evidence that the blood in the left side of the heart is cooler than in the right side, and the conclusion seemed to be justified that the blood in circulating through the lungs is more cooled by coming in contact with the air and by the loss of watery vapor than it is heated by any chemical processes which may have their seat in the lungs. It would, therefore, be expected that the breathing of warm air saturated with moisture would cause a rise of temperature of the arterial blood. The experiments of Lombard,* however, seemed to show that this was not always the case, and further doubts were thrown upon the correctness of the above view by the observations of Jacobson and Bernhardt† who, in a series of experiments on rabbits with thermo-electric needles, found that, in the majority of cases, the blood of the left heart was warmer than that of the right.

The question having been thus again opened, a large number of observations were made on dogs by Koerner and Heidenhain, partly with thermometers and partly with thermo-electric apparatus. They found that out of 94 observations only one showed an equality of temperature between the two sides of the heart, while in all the others the difference was in favor of the right side. The authors, however, propose an entirely new explanation of the cause of this difference. According to them thermometric measurements in the ventricles give not only the temperature of the blood contained in them but also that of the muscular walls, for these walls must necessarily at every systole be brought in contact with the bulb of the thermometer or the thermopile. Now the walls of the right ventricle are warmer than those of the left, because they lie in contact with the diaphragm which rests directly upon the warm organs of the abdominal cavity. The real reason then of the higher temperature in the right ventricle is that heat is conducted to its walls from the abdominal organs, while from the left ventricle heat is conducted away by the cooler lungs which surround it. This theory is supported by the observation that the difference of temperature between the two ventricles is increased by keeping the instruments pressed firmly against the muscular walls. Moreover filling the stomach with cold water or placing a bag filled with cold water under the diaphragm was found to diminish this difference, while the use of warm water in a similar way increased it. According to this theory, observations on the comparative temperature in the two ventricles have not the same physiological interest which has been heretofore attributed to them, since the difference of temperature depends chiefly on anatomical relations of organs which may vary in different animals.

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* Archives de Physiologie, 1868, p. 479.

† Centralblatt für die med. Wissenschaften, 1868, p. 643.

Since the publication of Koerner's and Heidenhain's experiments, Bernard has given a course of lectures on animal heat, where he has mapped out very minutely the variations of temperature in the different parts of the animal body. He discusses the above theory and rejects it on the ground that the observations of Hering on a case of ectopia cordis have shown that even in this malformation the temperature of the right ventricle is higher than that of the left. Here, of course, the possibility of the communication of heat between the heart and the neighboring organs is excluded.

Stricker and Albert are also unwilling to accept Heidenhain's explanation, for they find that the difference in temperature between the right and left ventricles is unaffected by opening the abdomen and withdrawing the viscera from contact with the diaphragm. These observers give a very extended series of measurements in all parts of the heart and great vessels, from which they conclude that the muscular walls of the left ventricle are warmer than those of the right, but the blood of the right ventricle warmer than that of the left. This would of course indicate a cooling of the blood in the lungs.

Even if the views of Heidenhain and Koerner should not be fully sustained by subsequent experiments, there is without doubt sufficient truth in them to deprive the question of a great deal of the importance which it was formerly thought to possess.

Localization of Cerebral Functions.

Fritsch und Hitzig.—Ueber die electrische Erregbarkeit des Grosshirns. Archiv für Anatomie, Physiologie und wissenschaftliche Medicin, 1870, p. 300.

Hitzig.—Ueber die beim Galvanisiren des Kopfes entstehenden Störungen der Muskelinnervation. Archiv für Anat., Physiol. und wissenschaftliche Medicin, 1871, p. 716.

Weitere Untersuchungen zur Physiologie des Gehirns. Do., 1871, p. 771.

Beaunis.—Note sur l'application des injections interstitielles à l'étude des fonctions des centres nerveux. Gazette Médicale de Paris, 1873, Nos. 30–31.

Nothnagel.—Interstitielle Injectionen in die Hirnsubstanz. Centralblatt für die med. Wissenschaften, 1872, p. 705.

Experimentelle Untersuchungen über die Functionen des Gehirns. Virchow's Archiv, 1873, p. 184.

Ferrier.—Experimental Researches in Cerebral Physiology and Pathology. British Medical Journal, April 26th, 1872.

J. Hughlings Jackson.—On the Anatomical and Physiological Localization of Movements in the Brain. Lancet, 1873, p. 84.

On the Anatomical Investigation of Epilepsy and Epileptiform Convulsions. British Medical Journal, May 10th, 1873.

Meynert.—Article on Brain of Mammals in Stricker's Handbook, and Résumé by Dr. J. J. Putnam. Archives of Scientific and Practical Medicine, i. p. 172.

Brown-Sequard.—On the Mechanism of production of Symptoms of Diseases of the Brain. Archives of Scientific and Practical Medicine, i. p. 117.

On the Sudden or Rapid Arrest of many Normal or Morbid Phenomena. Do., i. p. 87.

The phenomena of aphasia were at one time regarded by most physiologists as affording satisfactory evidence that at least *one* mental faculty has its seat in a definite, circumscribed portion of the brain. The accumulation, however, of recorded cases in which the disease has been observed without the lesion, and of those in which the lesion has been found without the disease (among which may be mentioned the celebrated "crow-bar case"), has gradually led to the abandonment of this view, as being, at best, only a partial expression of the truth.

The experiments of Fritsch and Hitzig have opened a new field of physiological investigation, and seem to furnish the best evidence we have of a localization of functions in the cerebral substance. These observers removed the upper portion of the skull of dogs, with the exception of a narrow strip of bone over the longitudinal sinus, and irritated the exposed surface of the brain with weak constant and induced currents. When the electrodes were placed upon the anterior portion of the cerebral lobes, muscular movements of various sorts were produced, which was not the case when the posterior portion was irritated. This is of interest, since, according to Meynert's anatomical investigations, the motor nerves are in communication with the anterior and parietal, and the sensitive nerves with the posterior and temporal portions of the brain. Fritsch and Hitzig further discovered that, by irritating certain definite portions of the convolutions, particular groups of muscles on the opposite side of the body could be brought into activity. In this way, "centres" were demonstrated for the muscles of the neck, for the extensors and adductors of the forearm, for the flexors and rotators of the arm, for the muscles of the foot and for the facial muscles. To control these results, the experiment was tried of boring through the skull of dogs at a point, determined by previous experiments to be the centre of movement of the right foreleg, and removing a small portion of the gray substance. Dogs thus operated upon were found to have only an imperfect control over the movements of the limb in question.

In this connection is to be noted a case of depressed fracture of the temporal bone, reported by Wernher, in *Virchow's Archiv* for 1872. Convulsion and paralysis occurred in those muscles whose "centres" were involved in the injury.

The observations of Fritsch and Hitzig have been confirmed and extended by Ferrier, who has, however, as yet published his results only in the form of a brief "preliminary notice." The following are some of the most important of his conclusions :—

I. Those groups of muscles which may be regarded as most voluntary, i. e., those which are in frequent and varied action, have particularly well-marked centres in the cerebral hemispheres. Thus, the centres for the muscles of the tail in the dog, for those of the paw in the cat and for those of the lips and mouth in the rabbit, were found to be very strongly pronounced.

II. The action of the hemispheres on the muscles is generally crossed, "but certain movements of the mouth, tongue and neck are bilaterally coördinated from each cerebral hemisphere."

III. The cerebellum contains centres of coördination for the muscles of the eye, injury to which causes loss of equilibrium.

This last conclusion seems also to be justified by the observations of

Hitzig on the effects of a galvanic current directed through this portion of the encephalon. If a constant current is directed from one mastoid fossa to the other, dizziness is produced, accompanied by apparent movements of external objects from the side of the anode to that of the kathode. These are due to real unconscious movements of eyes in the opposite direction. If the eyes are closed, the body seems to be falling to the side of the kathode, and an effort is made to recover the equilibrium, which results in a fall to the opposite side. The rolling movements of animals after injury to the cerebellum are explained by Hitzig on the supposition that the animal so affected has continually the impression that it is falling to the injured side, and consequently makes violent efforts which carry it constantly in the opposite direction.*

To avoid the general disturbance resulting from the removal of a portion of a nervous centre, Beaunis and Nothnagel attempted to destroy the functional activity of definite portions of the brain by the injection of various fluids. Nothnagel injected a concentrated solution of chromic acid through a fine subcutaneous syringe. The effect is to color green and to harden the cerebral tissue with which the acid comes in contact. Around this portion a very circumscribed inflammation is produced, and its functions may be considered as annihilated. Rabbits were found to survive this operation only from one to three weeks. It can scarcely be regarded, therefore, as simply annihilating the function of the part affected. The results of Nothnagel's experiments confirm, in general, those of Fritsch and Hitzig. The former observer, however, distinguishes the motor centre of the limbs from the centre of muscular sense. Both lie close together in the anterior portion of the opposite cerebral lobe, the former a little farther forward than the latter. The great advantage of Nothnagel's method is that it enables him to operate upon the deeper portions of the brain without destroying the more superficial parts.

Lesions of the brain are termed, by Hughlings Jackson, "destroying" or "discharging," according as they annihilate or stimulate the functional activity of the parts where they occur, the former causing paralysis and the latter convulsions, when occurring in parts of the brain connected with the muscular system. The study of destroying lesions of the corpora striata has shown that the paralysis thereby produced on the opposite side of the body attacks first the more voluntary and afterwards the more automatic muscles. The same order is observed in the case of destroying lesions which affect only mental phenomena, the brain, in undergoing slow deterioration, becoming more automatic and less voluntary in its actions. Something of the same sort seems to take place in the temporary mental paralysis produced by alcohol, thus affording a physiological explanation of the saying, "*in vino veritas.*"

"Discharging" lesions also show the same peculiarity in regard to the order of the muscles affected, convulsions beginning most frequently with the hand, less frequently with the face, and still less frequently with the foot.

Hughlings Jackson refers to Ferrier's experiments as artificially produced discharging lesions, and regards the study of discharging lesions, natural and artificial, as the only method by which the localiza-

* See, also, Report on Electro-Therapeutics, this JOURNAL, Oct. 31st, 1872.

tion of functions in the cerebral lobes can be determined; for though a destroying lesion may annihilate the functional activity of a certain portion of the brain, it is quite possible that its functions may be discharged by some other portion, and thus no effect of the injury be apparent. On the other hand, a discharging lesion, by bringing a certain portion of the brain into activity, must always produce some definite effect. The recognition of this power of vicarious activity in the different parts of the central nervous system will doubtless explain many apparent contradictions between pathological observations and physiological experiments; for it is quite conceivable that experimental destruction of a certain part of a nervous centre may cause serious functional disturbance, while pathological degeneration of the same part, occurring slowly and allowing time for other portions to assume the functions of the part affected, may produce no such result.

In the recently published views of Brown-Séquard on nervous physiology, the so-called "inhibitory" processes play a much more important part than has been heretofore assigned to them, twenty different kinds of inhibitory phenomena being enumerated, of which the arrest of the heart by the galvanization of the vagus nerve is the best understood. Both the production and the cure of a great variety of mental disturbances are due, according to this author, to inhibitory influences proceeding from one portion of the central nervous system to another. While admitting "that a considerable alteration of the whole extent of the cortical parts of the brain will affect the mental faculties in a direct way, i. e., as taught by the old theories," Brown-Séquard concludes, "from the study of every symptom of brain disease, that all parts of the brain resemble the peripheric parts of the nervous system, in being able, under irritation, to act on any of its other parts, modifying their activity, so as to destroy, or diminish, or to increase and to morbidly alter it." In other words, an injury to any one portion of the brain may produce not only the *direct* effects of a destroying or discharging lesion of the part affected, but also an *indirect* effect upon *any other* portion of the brain; and this indirect effect may be either to increase, to diminish, or to morbidly alter the functions of the part thus affected. With such unlimited possibilities in the way of effects, cerebral lesions may well be regarded as defying diagnosis.

Trophic Nerves.

Joseph.—Ueber den Einfluss der Nerven auf Ernährung und Neubildung. Archiv für Anat., Physiol. und wissenschaftliche Medicin, 1872, p. 206.

Power.—The Influence of the Nerves on Nutrition. Practitioner, March, 1873.

Charcot.—Leçons sur les Maladies du Système Nerveux. Paris. 1872.

Vulpian.—Recherches relatives à l'influence des lésions traumatiques des nerfs sur les propriétés physiologiques et la structure des muscles. Archives de Physiologie, 1872, p. 245.

Hayem.—Note sur deux cas de lésions cutanées consécutives à des sections de nerfs. Archives de Physiologie, 1873, p. 212.

Ranvier.—De la dégénérescence des nerfs après leur section. Comptes Rendus, Dec. 30, 1872.

The question whether the nerves exercise any *direct* influence upon

the nutrition of the tissues to which they are distributed is one of great practical importance; for, as Dr. Anstie well observes, "the hope of discovering scientific and intelligible rules for guidance in the employment of many important remedies, and especially *electricity*, depends largely upon our coming to a decision on this matter." Notwithstanding the great number of experiments and observations that have been made with a view of throwing light on this question, there is still a great difference of opinion amongst writers on the subject. Joseph concludes, as the result of his experiments on frogs, that in these animals the nerves exercise absolutely no direct influence on the nutrition of the tissues. Section of the nerves of a limb produces no disturbance of nutrition, excepting the slow atrophy resulting from functional inactivity, and irritation of the nerves is likewise without effect. Wounds heal as perfectly and as rapidly whether the nerves supplying the parts are divided, irritated or left intact.

Power also denies the existence of trophic nerves, and refers the nutritive disturbances which are known to frequently follow nerve lesions in man and the higher animals either to irritation or to paralysis of the vaso-motor nerves, or to a loss of functional activity of the part. He cites various instances of tissues, both normal and pathological, which are destitute of nerves, and where, nevertheless, nutrition takes place; but this, of course, is no proof that nerves may not influence the nutrition of the tissues in which they are present, any more than the absence of bloodvessels in the cornea is a proof that the circulation of the blood plays no part in the processes of nutrition.

Charcot, who discusses the whole question most thoroughly and systematically, rejects the theory that paralysis of the vaso-motor nerves alone can cause trophic disturbances, for section of these nerves in animals is not followed by any such results if care is taken to exclude all other possible causes of trouble, though the hyperæmia thus produced creates, without doubt, a certain predisposition to inflammations. Moreover, the lesions of the skin, muscles, joints, &c., which occur in man, as the result of injuries to nerves, are not accompanied, as a rule, by signs of vaso-motor paralysis or irritation.

"Nothing is better established in pathology," says Charcot, "than the existence of these trophic disturbances as the result of lesions of the nervous centres or the nerves. And yet the most advanced physiology teaches that, normally, the nutrition of the different parts of the body does not depend essentially upon an influence of the nervous system." To explain this apparent contradiction, Charcot accepts the proposition first announced by Brown-Séquard, that *section* of a nerve causes, in the tissues supplied by it, only those slow changes which are due to a functional inactivity of the part, while *irritation* of a nerve may produce, with considerable rapidity, a variety of lesions which have generally more or less of an inflammatory character. On this principle are to be explained the results of incomplete section, contusion, &c., of nerves, these injuries causing irritation of the nerve in consequence of the inflammation which they produce. This is quite in accordance with the views of Weir Mitchell, derived from the observation of gun-shot wounds of nerves.

On the other hand, the observations of Vulpian on the influence of nerve-lesions on the muscles which they supply, lead him to the conclusion that, as far as the effect on muscular contractility is concerned,

there is little difference between a section, an excision, a contusion, a ligature and a cauterization of the nerve. In this connection is also to be noticed a case, reported by Hayem, of section of the median nerve by a splinter of glass, resulting in loss of motion and sensation in the thumb, index and middle finger, followed, after six weeks, by a pustular eruption and ulceration of the ends of the fingers. As there was never any great pain, it seems unlikely that an inflammation of the nerve at the level of the wound could have been the cause of the morbid phenomena.

Whatever may be the result of section of a nerve on the nutrition of the parts to which it is distributed, there is no doubt that the peripheric portion of the divided nerve undergoes changes, usually described as degenerative, in consequence of which it loses its irritability. That these changes involve the destruction of the axis-cylinder is now generally admitted, but the way in which this result is brought about is still under discussion. Ranvier has recently investigated the subject in the light thrown upon the question by his observations on the structure of the normal nerve fibre.* He finds that, immediately after the section of the nerve, the nuclei of the peripheric portion enlarge and proliferate, and the protoplasm around them increases in amount in such a way as to press upon the medullary substance and axis-cylinder and, finally, to destroy their continuity. This happens about the third or fourth day after the section, which is about the time at which divided nerves are found to have lost their irritability. Ranvier considers that an influence proceeds from the nervous centres which regulates or controls the nutrition of the nerves and of the tissues to which they are distributed. Section of the nerve is therefore followed by an uncontrolled and irregular growth (often leading to destruction) of the parts thus deprived of nervous influences.

It will thus be seen that the question is far from being settled. It would at first sight seem improbable that section and irritation of a nerve should both produce the same effect upon the tissues to which the nerve is distributed; but it must be borne in mind that it is probably impossible to produce experimentally a simple withdrawal of the tissues from the influence of the nerves, for a section of a nerve, however carefully accomplished, is liable to produce a certain amount of irritation both at the time of section and in the course of healing. The observations of Ranvier, showing a similarity in the processes which take place in the peripheric end of a divided nerve and in tissues whose nerves are irritated, may be regarded as indicating that the so-called degenerative changes in divided nerves are really due to irritation at the point of section. Moreover, the fact that divided nerves, after undergoing so-called degeneration, are regenerated without reunion of the divided portions, seems to show that the changes in the peripheric portion cannot be due to a mere absence of an influence proceeding from the nervous centres. If this view be correct, there is only a difference in degree between the effects of section, and those of irritation, of a nerve; and while we cannot admit the existence of special nerve fibres charged with conveying trophic influences to the tissues, we must yet acknowledge that, in certain morbid states of the nerves and nervous centres, an influence is transmitted to the tissues in consequence of which disturbances of nutrition occur.

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